San Joaquin Basin Adult Chinook Otolith Analysis

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0269: San Joaquin Basin Adult Chinook Otolith Analysis

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

This proposal addresses an important gap in the knowledge of Chinook salmon management in the Central Valley of California. The applicants propose to analyze the otoliths (which were already collected from 1999 – 2002) from adult (spawning) Chinook salmon in the Sacramento – San Joaquin River Basin. They propose using micro–structural and micro–chemical analysis techniques to gain information on: the proportion of natural–produced versus hatchery–produced salmon in the river system; gain further insight into the dynamics of juvenile Chinook salmon migrations; assess the relative contribution of Chinook fry out–migrants to the adult Chinook escapement, and gain insight into the importance of environmental conditions (especially flow levels) on the migrations and survival of juvenile Chinook salmon. Attaining a reliable estimate of hatchery versus naturally produced fish in this river system is important for management of this fish stock, and is readily obtainable through the work proposed in this application. The other objectives could also have important implications for water resource management in this system (by guiding flow regime regulation for the benefit of juvenile Chinook outmigrants), but are technically more difficult to attain.

Additional Comments:

This proposal does address an important information need for the management of these fish stocks. While generally sound in reasoning, several valid, important technical issues were raised from two of the reviewers. Specific technical issues raised by those two reviewers can be read in their well written reviews (I especially suggest you read the review by Richard Bush). Richard Bush gave an excellent review of this proposal and his expertise in the field of otolith microanalysis provided many crucial suggestions which would be essential to the success of this proposal. The general concerns are: the applicants lack the technical expertise to carry out the micro−structural and micro−chemical analyses of the otoliths (and several important limitations of the proposal stem from this); not enough attention is giving to the gathering of information on the spatial and temporal distributions of the elemental/isotope compositions and water conditions in the river basin; the applicants lack credentials/experience in handling this type of technical data and have little to no history of publication; several elements of the proposed budget seem over−inflated while one component of the microchemical lab analysis seems unrealistically low. The overall suggestions on what to do with this proposal (from the reviewers): the general justification for this research is valid and the information gathered would be quite useful. Because of this, none of the reviewers wished to see the proposal completely abandoned. One suggestion was to fund this study for the initial year or so of the proposed work (see "first phase in Executive Summary), allowing the feasibility of the techniques to be evaluated, and allowing the applicants to demonstrate their ability to perform the tasks and deliver the products in this proposal. The other main suggestion was to fund this study, in a slightly expanded form, for a longer period, but contingent upon the applicants acquiring collaborators who are highly skilled/experienced in the field of otolith micro−structural and micro−chemical analysis. A reasonable proposal with some serious technical deficiencies but high scientific value. Expected to add solid basic knowledge/understanding of the topic proposed.

This proposal addresses an important gap in the knowledge of Chinook salmon management in the Central Valley of California. The applicants propose to analyze the otoliths (which were already collected from 1999 – 2002) from adult (spawning) Chinook salmon in the Sacramento – San Joaquin River Basin. They propose using micro–structural and micro–chemical analysis techniques to gain information on: the proportion of natural–produced versus hatchery–produced salmon in the river system; gain further insight into the dynamics of juvenile Chinook salmon migrations; assess the relative contribution of Chinook fry out–migrants to the adult Chinook escapement, and gain insight into the importance of environmental conditions (especially flow levels) on the migrations and survival of juvenile Chinook salmon. Attaining a reliable estimate of hatchery versus naturally produced fish in this river system is important for management of this fish stock, and is readily obtainable through the work proposed in this application. The other objectives could also have important implications for water resource management in this system (by guiding flow regime regulation for the benefit of juvenile Chinook outmigrants), but are technically more difficult to attain.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

San Joaquin Basin adult Chinook otolith analysis

The proposal raised some good questions, but it has serious technical issues. The panel ranked this proposal as inadequate primarily due to an apparent lack of expertise in the investigator team with the methodologies required for the study. There was a clear lack of demonstrated experience in the published literature record as well as apparent lack of understanding of the limitations of techniques cited and proposed for use in this study. The investigators failed to demonstrate solid competence in the methodologies required for the study. The panel also identified problems in the proposed protocol that would bias, if not result in incorrect analysis of results. For example the otoliths that would be analyzed

were collected during both low and high water years, but no concurrent water sample was collected. The investigators also have proposed too many hypotheses that could be reasonably tested within the scope of the project. The budget was considered over−inflated. It was recommended that collaboration with other technical experts in otolith analysis, possibly at UC Berkeley, would be advantageous in refining the objectives and scope of this study.

Final Ranking: Inadequate

Technical Review #1

proposal title: San Joaquin Basin Adult Chinook Otolith Analysis

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals are clearly and logically presented. The idea is quite interesting and is a timely application of a fairly novel technique.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The justification is sound and builds very well on existing knowledge. The results from this work will help fill critical gaps in our understanding of salmon migration dynamics and also further the applications of otolith microchemistry.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	

	Straightforward and logical approach. The internal controls are adequate to eliminate ambiguitiy of the results and are likely to yield significant findings. The project has a high probabilityof generating new knowledge about the use of otolith microchemistry to field situations and will provide extremely useful data to assist in the management of water flow to maximize salmon stocks.
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**Given the straightforward nature of the work and the vast amount of existing otolith samples, this work has a high likelihood of success.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Interpretation of the data depends on the results of the microchemistry analysis. But experimental design is likely to yield good associations between the different parameters.**
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Highly likely to produce timely and useful information for overal management of the Delta and salmon stocks.**
Rating	**excellent**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**PI's have solid background and tchnical expertise in this area. Only weakness is the lack of peer-reviewed publications among the participants.**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The nature of this work is very labor-intensive and the budget relfects this. It is reasonable and adequate for the amount of work proposed.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**Good, solid project that uses existing resources (otolith samples) and should produce the type of information needed for sound poicy and management decisions.**
Rating	

	excellent

Technical Review #2

proposal title: San Joaquin Basin Adult Chinook Otolith Analysis

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals are very timely and extremely important. The goals are to quantify: 1) the contribution of emigrants from different life stages (fry, parr, smolts) to Chinook salmon escapement in three different San Joaquin tributaries, 2) the contribution of hatchery supplementation to natural spawning production in these three tributaries, and 3) determine the impact of different flow conditions in the three tributaries on the relative contribution of immigrating fry (as opposed to parr or smolts) to Chinook escapement and reproduction. The applicants intend to analyze otolith microchemistry (including stable isotope analyses) and otolith microstructure to accomplish these goals. They state the intention to demonstrate the applicability of these techniques towards accomplishing these goals; yet, the applicability and efficiacy of these methods has already been "demonstrated" (albeit, recently). They also, state that this project will continue until a more intensive hatchery marking program (in development) begins to produce data that will allow analysis of hatchery supplementation efficacy and migration timing. However, the otolith microchemistry/microstructural analyses they propose are potentially much more powerful than hatchery marking and could be used to address a much wider range of questions, if implemented on a wider scale. In other words, if implemented on a more intensive scale than described here, this project would obviate**

	the need for the planned expansion of hatchery marking efforts.
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study appears to be well−justified. From the spawning grounds through the confluence of the San Joaquin and Sacramento Rivers (the "Delta"), water management strategies have been modified in order to increase survival of juvenile salmon. Yet, little is known about the migratory strategies of juvenile Chinook from the San Joaquin tributaries and even less is known about how these migratory strategies are influenced by flow conditions. Finally, nothing is known about how these migration strategies, and their interaction with environmental conditions, influence later life−history strategies (estuary residence periods, ocean residence periods, straying upon return to freshwater as adults). The applicants' propose only a small scale study; but, if their plans were implemented on a larger scale (more fish, more years), the data collected would be extremely valuable to scientists, restoration ecologists, and water managers. As proposed though, the study will be capable of little more than "demonstrating" the efficacy of these techniques and the abilities of the project team (see below).** **The authors mention an upcoming hatchery−marking program that will produce more information than previously marking efforts. CalFED and its partner agencies should search for a way of directing funds away from intensive hatchery−marking programs and towards a project like the one proposed here (but, see below for reservations I have about this particular**

	program). The hatchery-marking program could be implemented as a pilot-scale ground-truthing supplement to an otolith micro-chemistry/micro-structural survey not the opposite, as proposed here.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach is potentially very powerful. The applicants will be able to determine which life-stage salmon are in when they begin their migration towards salt-water. By combining this information with known age-size relationships, the authors will be able to determine the size, age, and calendar timing of migrations. This has big implications for water management in the San Joaquin River.** **The applicants will also be able to identify the onset and duration of brackish water residence because the elements and isotopes they will measure are sensitive to changes in salinity and temperature. The applicants plan to measure elemental and isotopic composition of water samples along the migratory route of juvenile samples and this will refine their estimate of each salmon's geographic location as it grows during migration. These results have significant implications for water management (i.e., water export) operations in the "Delta". This is true even though the salt-water signature will not appear until salmon have migrated past the "Delta" because freshwater flows are managed to limit saltwater intrusion to areas well-west of the "Delta" (which is not really a "Delta" at all but a confluence). The applicants should be able to back-calculate to determine a period**

of exposure to water export operations.

Otoliths also record whether fish originated in hatcheries or were naturally spawned. These methods will probably reveal each fish's stream of origin because the underlying bedrock in the different drainages may differ enough to produce different isotopic signatures. Again, this is valuable information because it will allow researchers and managers to determine the relative contribution of hatchery fish to the spawning population. These techniques could also reveal valuable information about straying of natural spawners between different natal streams – a parameter that could be important in the management of these three populations.

Although the approach is potentially powerful, these are new and subtle techniques. The data (particularly the stable isotope and elemental ratio information) require the interpretation of those trained in the methodology. The applicants do not seem to have much background in interpreting micro−chemical and micro−structural data from salmon otoliths. One of the PI's has some experience reading rockfish otoliths but there is no evidence that this individual is experienced enough to interpret variations in the chemical signature of salmon otoliths.

Also, accurate interpretation of the data requires reference to known standards. The applicants plan to analyze water samples from the tributary streams, the San Joaquin, The "delta" and the "Bay". This is a good idea. In addition, they should rear salmon (which could be obtained from a hatchery) in each of these environments to determine how the water chemistry affects the actual composition of the otolith, as this is not always straightforward. The applicants plan to measure water quality in three locations in each area (i.e. each stream, the mainstem, the "delta", and "the Bay). Clearly, they will lose spatial resolution as they move downstream. They do not explain the

reasoning behind this changing resolution.

Similarly, the authors will be able to increase the power of their conclusions if they combine their otolith results with data on outmigration. Regardless of the final composition of the spawning class (regarding the contribution of hatcheries or the contribution of outmigrating fry etc), the researchers will not be able to measure the efficiency with which any class of salmon juveniles contributes to spawning unless they can estimate the number or individuals that belonged to that class as juveniles. For example, if hatchery fish are 10% of the spawning class, is that good or bad? If fish that outmigrated as fry make up 10% of the spawning class is that a lot or a little? In each case, the answer can only be derived by measuring the relative contribution of each "class" to the outmigrant populations.

The authors plan to examine fish that were spawned in three different years. These years varied in outflow conditions. The applicants hope to relate differences in emigration behavior to differences in flow conditions. They do not acknowledge that their sample size will be not much more than N=1 for each water−year type. No matter how many fish they sample, all of them were hatched in the same year and thus were exposed to the same ocean, estuary, fishing conditions, etc. Also, the authors assume that all Chinook follow either a 3−year or 4−year life cycle. One of the hypotheses tested in this study is that differences in hatching/rearing conditions (such as freshwater outflow or hatchery−rearing) produce different life−history distributions. For example, fish that grow faster are likely to return to spawn earlier than slow growing fish. There are also density−dependent affects that could produce changes in the age−structure of the spawning population. The authors limit their ability to detect such changes in spawning class structure by limiting the years in which they look for returning spawners. Are there no

	“jacks/jills” (two year old spawners) or 5–year old spawners in this population? **Finally, the power of these techniques to detect meaningful life–history shifts and relate those back to freshwater flow conditions is limited by sample size. As noted above, the applicants will have only one example of each water–year type to compare. Statisical analyses will not be possible. The authors plan to use otoliths from 75 fish in each of the tribuataries in each year. The applicants hope to analyze differences between several different groups: emigrating fry, parr, smolts; adults that return at 3 years v. 4 years; males and females; hatchery and wild spawned. After all these sub–groups are analyzed for consistent within–group patterns, the sample sizes are fairly small (**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The project is relatively well documented and feasible. The question is: is this team capable of performing this project? This is complicated research and the applicants have not demonstrated a good track–record of publishing complicated research. They have no experience producing or interpreting micro–chemical data or stable isotope data. The scale of the project is manageable but should be expanded (see APPROACH above) to really justify the expense. Collecting more otoliths in more years won’t actually increase the cost of the project that much but it will improve the value of the results immensely.**
Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**This is a monitoring project. One "treatment" applied is different flow regimes. It will be difficult to evaluate the impact of flow regime on the outmigrant−survivorship relationship. Intensive monitoring of outmigrant juveniles should be added to the study design. Increasing the duration of the study will be necessary to deal with the effects of temporal autocorrelation. The CBDA grant period is usually three years long, so this justifies the duration proposed for this project; but, the applicants have made it seem as though they will stop this project after the project−period because a more intensive fish−marking project will come on−line in the near future. The project is less attractive if it will be dropped after three years.**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The data generated from this project are potentially extremely valuable. Even given its presently limited scope, the project could produce interesting results. However, the project team has not made it clear that the results will be widely−distributed or**

	peer-reviewed.
Rating	very good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	I am doubtful of this team's capabilities. Only one member of the team has any experience with the major techniques involved and that person has only one peer-reviewed paper. He also does not appear to have much experience with the type of data the team plans to produce. The poor publication record of this team, the paucity of peer-reviewed literature cited in their proposal, and the lack of suitably trained personnel suggest this team may not be able to deliver. The somewhat sloppy presentation of this proposal suggest a lack of attention to detail that will be crucial to the success of this program.
Rating	fair

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget is well-described and detailed. My major concern is that a very large fraction of the budget is dedicated to project management, data management, and discussion of preliminary results (meetings, symposia, etc.) compared with a relatively small amount of money dedicated to actually gathering, analyzing, or preparing data for publication in peer-review journals. The applicants have requested substantial

	funds (over 10% of the budget) for coordinating with stakeholders and attending workshops; but, they have allocated only 4 hours each for two of the PI's to work on submitting articles to the peer-reviewed literature. At the size they have presented the project, the products are too expensive; if they expanded their program as suggested (and cut back on the amount of time and $$$ spent in conferences/workshops/meetings) they could produce EXTREMELY valuable results.
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**I think the goals and methods of this project are essential to CBDA and AFRP. It is difficult to imagine that wild salmon populations will be maintained and restored in tributaries of the San Joaquin (especially with increased water demands and the decreased supplies that will return during the next drough cycle) without the type of information a project of this type could generate. That said, the scope of work proposed here is too small, the project team too inexperienced (with this type of data), and too lax about exposing their results to scientific peer-review (by publishing) to make the investment.worthwhile. If the applicants came back with a proposal for this work that had a greater temporal extent and collaborators who have experience a) with otolith microchemistry and microstructure and b) publishing their results, I would recommend funding. I cannot recommend funding of the proposal as currently proposed**
Rating	**fair**

Technical Review #3

proposal title: San Joaquin Basin Adult Chinook Otolith Analysis

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**This proposal states that the goals of this Central Valley study are twofold: 1) to provide resource managers with critical biological and ecological information to make informed decisions regarding water and salmon management, and 2) to provide resource managers with the means to evaluate the success of CALFED water management and habitat restoration efforts. The proposal authors, Sonke et al., hypothesize that through the use of "innovative" otolith methodology they will adequately address the annual wild salmon contribution to the San Joaquin watershed escapement, the effects of streamflow and environmental conditions on juvenile fish contribution to the overall run, and the proportion of wild versus hatchery fish spawning in the Basin. The objectives of this project remain internally consistent throughout the proposal. Although, the objectives presented appear to be out of order. This study would benefit if the investigators had provided a clear description of their water sampling sites (in lieu of saying "Delta, mainstem, San Joaquin, Stanislaus, Tuolumne, and Merced rivers") and had scheduled to do a pilot study during the first year to verify the water chemistry at the sites they chose differ enough to justify the proposed work. This would enable Sonke et al. to use adaptive management to plan for subsequent field seasons after determining potential problems (e.g. not allowing enough time to prepare and analyze the large number of otoliths) and unforeseen complications (e.g.**

	Elemental Research Inc. analytical delays) to ensure success in the second and third years. The idea of being able to distinguish between hatchery and wild fish is extremely important in the context of obtaining a better understanding of the overall life history of Central Valley chinook salmon and maintaining a healthy, viable population. This research is timely because it will enable fishery managers for the first time to estimate the contribution of hatchery fish to natural production. Much of what is presently known about the Central Valley fishery is based on hatchery fish returns or results drawn on sampling a mixed population of both wild and hatchery fish. In a time when there is a large degree of uncertainty and skepticism on whether to classify hatchery and wild fish as one and the same, or as separate stocks, we need to carefully design studies that will help answer this long−standing debate. One problem I see with the hypotheses being tested by Objective 5 is that a clear distinction is not made between the terms "naturally produced" and "wild stocks". The terms are used interchangeably throughout the proposal and that is incorrect. Naturally produced salmon simply refers to fish that spawned in a natural stream channel, while "wild stocks" refers to fish that are genetically distinct and endemic to a particular watershed or ecologically significant unit (ESU). It is true that wild stocks are naturally produced, unless they are being propagated in a captive broodstock conservation hatchery. But it is a mistake to classify naturally produced fish as wild stocks since truly "wild" fish can and do interbreed with hatchery fish on the spawning grounds. A distinction needs to be made here because the otolith S34 technique will only be able to determine fish that were raised in a hatchery (due to the marine−based diet), it will not be able to account for the hatchery contribution to the wild population.
Rating	**good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**This study is testing a justifiable question that should provide a better understanding of the relative contribution of hatchery chinook salmon to the overall escapement in the San Joaquin Basin. The existing knowledge is based on a hatchery marking program that marks less than 10−15% of all hatchery salmon. Returns from this marked group are too low to make confident estimates of the contribution that hatchery fish have on the overall escapement. A conceptual model of the chinook salmon life cycle is included with the proposal. Because the model doesn't directly reference any of the five objectives or 20 different accompanying tasks, I found this section to be one of the weaker parts of the proposal. The model does not serve its purpose of facilitating the reader's comprehension of the underlying basis for the work that is proposed. It is almost as though this model was included in order to satisfy a proposal requirement. This observation is based on the fact that the model was borrowed from another source and was not modified to assist the readers' understanding of how this experiment will adequately test its objectives. I feel a conceptual model outlining the expected responses to the proposed experimental design is necessary to demonstrate how the otolith results will be interpreted and related to the environmental conditions measured. The model I suggest would be especially useful not only to the reviewers of this proposal, but also to the researchers to help guide their project based on the results from the exploratory first year of data collection and interpretation. Justification is lacking in this proposal to fund this study as a full−scale three year implementation project due to the uncertainties that exist with the study methodology. Sonke et al. draw on**

	numerous otolith publications to justify the underlying rationale for this project, but fail to provide any water chemistry data or pilot otolith data of their own to show that they are capable of delivering interpretative results from otolith microstructure and microchemistry for some 900 fish. Therefore, it is unclear why they haven't determined this project to be a pilot/demonstration project. In reviewing this proposal, I found it would have been very helpful to have had a map of the study area marked with all water and fish sampling sites. At the very least, they need to provide a detailed methodology of how they will decide on where sampling will occur and demonstrate how they will attempt to characterize the meromictic conditions that commonly occur in estuarine waters where warm freshwater rivers and cool bay waters meet. If there are deepwater habitats where fish could travel through different water chemistry profiles just by moving up and down through the water column it would make differentiating between delta, estuarine and ocean entry difficult to pinpoint.
Rating	**fair**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The research team is proposing to apply numerous otolith methods to measure the contribution of hatchery fish returning to the San Joaquin watershed and to identify other trends in biological characteristics of the adult population. Parts of their study plan are based on published results and other aspects are based on personal communication with other researchers in the field of fish otolith research. Sonke et al. have designed their study in**

part after Titus et al. 2004, who utilized otolith microstructure to study chinook salmon migration/movement in California's Central Valley waterways. Sonke et al. indicate that they will apply the Titus et al. 2004 methodology to distinguish between adult hatchery and wild fish. The team also drew on the experience of Rachel Barnett–Johnson who has worked with Central Valley juvenile chinook salmon in developing otolith metrics to distinguish hatchery fish based on structural based information. Another previous Sacramento River study indicates that hatchery reared chinook salmon fry have an elevated S34 signal at their otolith primordia (i.e. earliest development), while the wild fish lack this signal (Weber et al. 2002). The success of this project will not only dependent on how well the team will be able to reconstruct the life–histories of these fish in a timely fashion, but will also hinge on the team's ability to associate environmental variables with the qualitative data they extract from the salmon otoliths. Otoliths are proposed to be used to satisfy numerous project objectives, including stock identification, reconstruction of environmental/migrational history, back–calculations of fish length at age, and use of microstructure to identify check marks. Many of the otolith methods proposed for this study are sound in methodology, but numerous inconsistencies about otolith applications and analytical techniques appear throughout the proposal. Sonke et al. appear to be new to the otolith research arena and unaware of some of the basic background information about otolith formation and interpretation. An example of this is demonstrated by the authors citing the Titus et al. 2004 study to guide their identification of "otolith microstructure methods . . . to distinguish between [adult] hatchery and wild fish". Titus et al. used otolith microstructure to estimate growth rates of juvenile chinook salmon. The problem in applying the Titus et al. methodology to adult otoliths is that the ability to detect otolith microstructure is limited to

approximately the first year in the life of salmonid fishes, after that the early otolith growth increments become occluded, if not impossible to interpret. A lack of knowledge in regards to the proper otolith microchemistry approach is also evident. The researchers indicate in the executive summary that LA−ICP/MS "will be used to quantify specific elements", which is correct; but then incorrectly indicate that the same analytical equipment will be used to obtain isotopic ratios (i.e. 87Sr/86Sr). The detection of the spatial distribution of isotopic ratios in otoliths is accomplished using an ion microprobe, PIXE or MCICP−MS. There is too much interference in a Laser Ablation ICPMS such that the doubly charged ions (e.g. Ca++/Ca+) and oxides (e.g. THO 248/TH242) cause sensitivity and stability problems when measuring high resolution isotopic data. The Multi−collector ICPMS alleviates these interference problems and allows multiple isotopic ratios to be determined for individual elements. In addition to methodological concerns, the "Personnel" section of the proposal does not indicate that any of the project personnel have experience working with salmonid otoliths. If any of the primary staff had prior experience reading chinook salmon otoliths, then this would be less of a concern as they could train and closely monitor a second reader. The proposal does not indicate this is the case. The experience level of the otolith reader (Mr. Chapman) is not a primary concern since he has experience working with rockfish, but a substantial amount of time will be necessary in order to be able to identify the "landmarks" they are hoping to use for navigation. No mention was made in how training will be accomplished (e.g. using a known−age otolith set, hatchery fish check identification, etc.). While reader accuracy error can be minimized by use of an otolith reference collection, precision error is commonly reduced (improved) by resolving interpretation differences among readers. This process is commonly called a "double−blind" study in which two experienced otolith

readers read randomly coded samples, and their reads are compared. In the event of >10% disagreement between readers, samples are re−read and if the error is not resolved the samples are not included in any further analyses. This proposal indicates that only one person will be conducting the otolith reads and plans to use a reference collection for quality control are not mentioned. Many fish are difficult to age and precision errors are always inherent at some level, therefore, Sonke et al. are exercising poor judgment in not designating a second otolith reader to help eliminate errors in the interpretation of data that will be vital to the success of this project. The potential results of this study are interesting, but are limited in their ability to benefit water resource management decisions. This study has more to do with aiding fishery managers than it does in benefiting how water management and habitat restoration may influence salmon escapement. Because this study is designed to investigate life history strategies over a relatively short time period (just shy of encompassing an entire chinook cohort) it will be difficult to extrapolate the significance of any study findings in determining the relative contribution of restoration actions as outlined in the "AFRP Goals" section. Instead of trying to answer so many different questions, this proposal would be stronger if it focused on what I see as its most important goal (determining hatchery versus natural contribution), while possibly adding an additional complimentary objective each consecutive year. While reading this proposal I found it difficult to keep all the different objectives and tasks separate, as did the author at times. An example of this occurred in Tasks 3.3 where the proposal states that "a spatially and temporally resolved 87Sr/86Sr profile . . . will take place simultaneously with the chemical analyses in Task 3.2". These two different analyses (Sr isotopes vs. elemental data) are being lumped together giving the reader the impression that the data is obtained with a single analysis using a single piece of analytical equipment. To begin with,

	it is not possible to collect elemental data and isotopic data using LA−ICPMS simultaneously. As I indicated earlier, isotopic ratios (87Sr/86Sr) require MC−ICPMS. In order to allow the sample chamber to equilibrate between otolith spots, MC−ICPMS requires three times the amount of washout delay compared to LA−ICPMS to reach background levels. This is important because one otolith being analyzed for 87Sr/86Sr takes 3 times the analysis time for one being analyzed for elemental data. This equates to MC−ICPMS analyses costing 3x more.
Rating	**fair**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**There are certain aspects of the proposed study design which are well documented (e.g. otolith source, need for better hatchery fish contribution estimates, otolith 34S method), while numerous uncertainties exist with other project objectives. Below is my review of the feasibility of the different tasks proposed, listed in order of Task ID number. I. Project management: There is no reason to believe that CDFG will not be able to handle this contract. II. Otolith acquisition/preparation/microstructure analysis: The fact that the otoliths have already been collected demonstrates that the collection of field samples will not be the limiting factor for this study. This is an advantage when working in an unpredictable environment. Furthermore, the only doubt remaining on how large of a sample size that will be available is only limited to the number of unusable samples or sample prep complications. Figure 4 indicates that 3**

months will be devoted to otolith preparation in 2006, which sounds sufficient for the 225 otoliths slated for that year. Even though the staff will be more experienced in the second year, the have scheduled a very optimistic goal of preparing a minimum of 675 otoliths in only 5 months. It always takes longer than planned to create thin sections that are high−quality enough for microstructure interpretation. Approximately the same amount of time is necessary to read the otolith microstructure as is needed to prepare them. Three and five months have been allotted for microstructural analysis in years 2006 and 2007 respectively. Once again, I think the time allotted for year 2006 is sufficient, but I believe 5 months is a gross underestimation of the time necessary to analyze 675 otoliths. Add a second "blind reader" into the equation, and they will need to double the time allotted or purchase to Image Analysis microscope set−ups. III. Determine elemental &isotopic composition of water/otoliths/hatchery contribution: The collection and analysis of the water will not be the problem. The biggest problem with the proposed study plan is that the water is not being collected in the same year as the otoliths. In order to carry out this study properly, the water collection would have begun back in 1999 to characterize all river, delta and estuarine habitats that the juvenile salmon (corresponding to the adult otoliths collected for this study) would have migrated through en route to the ocean. The 1999 cohort would have returned to spawn in 2002 at age 3, 2003 at age 4 and 2004 at age 5. Using fish age and birth year, the researchers would have been able to match it up with the water chemistry for the representative year. Without collecting the water chemistry data for any single year that

can be matched up with the corresponding otolith chemistry presents a major impediment for drawing conclusions to this objective (e.g. mainstem river residence, delta arrival, estuary/ocean entry). Numerous publications using the Otolith elemental fingerprint methodology (Campana et al. 1995, Gillanders and Kingsford 1996 &2000; all cited in this proposal) stress the point that if a comparison using adult otolith chemistry is being made then it is necessary to have water chemistry for when the fish was a juvenile because differences exist from year to year. The same environmental conditions being tested in Task IV are what can create the wet or dry periods that may influence estuarine waters and therefore otolith microchemistry among years. IV. Environmental Variable Assessment: The environmental data will be easily obtained from USBR, USGS, DWR, and DFG monitoring stations. These cursory analyses may provide insight, but will not provide conclusive evidence regarding the hypothesis that fry and parr outmigrant contribution is related to a particular environmental variable. V. Estimate Hatchery Contribution: The otolith 34S method is clearly defined and documented and is being applied in a technically sound manner. The one problem I have with this task is that Task 5.2 &5.3 are both examining the same question. It makes sense in the first year when working with the smaller otolith set to employ both methods to see which differentiates hatchery and wild stocks the best, but there is no mention that this is what is being planned. Even a just a subset (n=100) of the otoliths from the first year would suffice for this comparison. There is no reason why 900 otoliths should be analyzed for microstructure (very time intensive) to distinguish stock identifications when 34S accomplishes the same

	task. Even more importantly, if microstructure analysis yields similar classification success to the costly 34S lab technique, then $90,000 of analytical expenses could be avoided. My rating is based on funding only the first phase of the project as a demonstration project.
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	It is quite possible that there are critical rearing habitats in the delta/estuary for Chinook salmon emigrating as sub yearlings from the San Joaquin Basin. Delta and estuarine habitat types are often overlooked in the salmonid life history and thus not included as possible preferred rearing habitats. I like the inclusive approach to investigate this question of residence patterns and growth rate performance during the chinook salmon freshwater/estuarine life−history. This study design combining otolith chemistry and otolith microstructure may provide evidence on what habitat types are most important in the juvenile lifecycle prior to ocean entry. Using this combination of otolith techniques,

	comparisons can be made between fish that migrated slowly through the riverine environment, those that chose to reside in delta habitats instead of the mainstem river, and those that rapidly migrated through the riverine habitat opting to rear in a brackish/estuarine environment. Possibly the most interpretive outcome of this study may be the first decent estimate of hatchery contribution to the San Joaquin watershed escapement. It is likely that one of the two methods (otolith 34S or microstructure) will prove to be a valid method in differentiating between naturally produced spawners and hatchery supplemented fish. If so, state and federal hatcheries may be able to cut operating costs by adopting otolith methods instead of the mass marking of juvenile fish prior to release.
Rating	**good**

Additional Comments

Comments	**The abstract watershed and delta boundaries discussed in the proposal are uninformative to this reviewer who does not work in the Central Valley. A map marked with study sites would help orient the reviewer with the study system and experimental design. It would have been nice if the authors would have been up front and addressed the fact that they will be collecting the water chemistry in different years than the otoliths. Because it wasn't mentioned almost makes me think they are unaware of the problems that will arise from not having real habitat characteristics (i.e. water chemistry) to match the fish up with. I find it odd that Sonke et al. make the case for a comparison of migrational cues based on drastically different environmental conditions in river flow during the study period, but they fail to realize that those same conditions will affect their ability to reclassify adult fish to their juvenile rearing habitats.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The Primary Staff is composed of a solid team of scientists that has extensive large-project management experience. The proposal is well-written in regards to the area of specialization of the different team members. Both Marston and Demko have extensive experience working with anadromous salmonids. Marston has been working for the CDFG San Joaquin River Anadromous Fish Research and Restoration Project for the past six years, and contributes on numerous other agency projects. Demko has been with the SP Cramer as a senior fishery biologist for eleven years working on predominantly on smolt migration issues. The infrastructure within the CDFG and the project oversight by the co-project leaders lend strong support for project success, but the lack of technical expertise regarding salmonid otolith application and a sound methodological approach to determine fish residence patterns are areas of concern. This lack of experience in using otolith microchemistry in monitoring the migratory behavior of salmon habitat residence is apparent from the inconsistencies and false assumptions in the proposal. While the team has substantial knowledge of smolt migration and monitoring environmental variables which is evident by Figures 2 and 3 and accompanying text; it is evident that their experience using otolith microstructure and microchemistry is limited (1 person w/ limited experience) to what they have read from the otolith literature (26 citations presented).**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget appears to adequately represent the work outlined in the proposal. The only equipment requested to carry out the study which is not already owned by is the image analysis equipment and the otolith preparation supplies. There are two parts of the budget that I disagree. The first is the request for almost $30000 to attend conferences and workshops over the course of the three year study. These necessary functions are a good way to inform and get feedback, and quite expensive to attend. Nevertheless, $10,000/year seems a little excessive. The other part of the budget that seems a little off is the cost of the otolith analyses. I feel that: • Task 3.2 ($135,000) $150/fish to analyze for an entire suite of elements – Fair • Task 3.3 ($9000) $9/fish to Quantify 87Sr/86Sr at the Core – Seems unrealistically inexpensive, most likely will analyze at least 3 spots at the core, and MC−ICPMS requires ~3 min between sample spots, with orientation time will end up taking ~ 20 minutes/fish. The standard rate for MC−ICPMS is ~ $175/hr??? This doesn't add up! • Task 5.1 ($90,000) $100/fish to analyze 34S along 15 spot transect – Fair The grand total for all otolith analyses comes to $234,000 as described in this proposal. I believe a more realistic price will be closer to $300,000. Possibly higher if the 87Sr/86Sr transects will also be done using the MC−ICPMS. The proposal indicated these analyses would be done using LA−ICPMS, but a description was provided earlier indicating why this cannot be done.**
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**Below is a bulleted list of pluses (+) and minuses (−) I used in evaluating my overall review of this proposal.** **+ Primary staff is highly experienced and capable of managing large−scale projects. + Addresses a question that will give fishery managers first measure of natural production + Project team has experience working with salmonid migratory behavior + May provide a better understanding of the relative importance of flow impact on fry dispersal − Modifications in the experimental design are necessary to maximize study effectiveness − Authors don't clearly link how their research may lead to restoration project assessment − Limited experience conducting otolith research, especially otolith microchemistry − This study would best be implemented as a demonstration project or trial pilot study first − Project should focus on fewer objectives, w/ emphasis on the hatchery vs wild component − Study design should match water collection to the habitats the juveniles are living in**
Rating	**fair**